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INJURY OF THE ARM COMMON TO CHILDREN OF ONE TO THREE OR FOUR YEARS OF AGE.

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FEB. 2d, 1862. A nurse, holding the hand of a little girl twenty-eight months old, in a moment of impatience gave it a sudden twitch. The child immediately cried out, would not allow herself to be touched, and held her arm motionless. Her mother, however, placed it in a sling, and three hours after the accident the limb was found in the following condition, viz., bent at an obtuse angle and resting against the body, the fore-arm much pronated. There was no apparent swelling or deformity, but the slightest motion was extremely painful, and the little patient was unable or unwilling to hold anything in her hand. On manipulating the limb there seemed to be some impediment in its natural movements, and in attempts to produce certain of these a feeling suggesting crepitus was detected, but before either the seat of this, or the actual diminution of mobility could be determined by examination, the abnormal condition of things vanished, the child ceased to cry, took her playthings in her hand, and the limb was evidently once more in its natural condition.

Feb. 7th, 1862. A little girl, thirty months old, fell whilst walking across the parlor floor, her mother holding her by the hand. When seen, her limb presented a condition precisely analogous to that described in the preceding case—there was the same pronated and semi-flexed position, the child was unwilling to use it and cried the moment it was touched. Before I could ascertain what the exact injury was, manipulation had produced the obscure crepitus, or something which seemed like it, the pain and immobility instantly disappeared, the child readily took its playthings, and with the exception of a little fright was herself again.

These two cases occurred in the course of the same week, and were the first and only ones of the kind which had ever fallen under my observation. I had considered them both as instances of incomplete dislocation of the head of the radius forward. This accident,

or, at all events, this phase of it, is not described in the common works on surgery, although a thesis in French, one or two articles in journals, and a description by two or three French writers may be found, in all of which it has been looked upon as some form of dislocation of the upper extremity of the radius. M. Goyrand, of Aix, however, who has written several articles on the subject, takes an entirely different view of the matter. In a paper recently presented to the Surgical Society of Paris (L'Union Médicale, Nov. 23, 1861), and which has fallen under my notice since the occurrence of the two cases above described, he sums up the symptoms of this accident so accurately that I venture to quote his description, believing that, in the absence of any readily accessible account of the injury, its recapitulation, with an abstract of some of his remarks, will not be altogether amiss.

A child, from one to three or four years of age, is saved from a threatened fall by being suddenly caught by the wrist, or it is lifted over a gutter, or made to step up by a violent pull of the pronated hand. At the moment of the strain, the person holding the hand notices a slight snap or shock. The child cries out and continues to complain; the limb hangs motionless by the side, a little in front, with the elbow partly bent and the hand pronated. Any attempt to supinate the hand increases the cries, the rotation outwards is brought up by a mechanical obstacle, and if this is not overcome, as soon as the hand is let go it returns to its pronated position. This symptom is pathognomonic. If the resistance is overcome, a snap is heard or a slight jar is felt, pain ceases, motion is restored and no trace of the accident remains. If nothing is done by the surgeon, after a certain period which varies from a few hours to several days, the limb spontaneously resumes its natural condition, ordinarily during sleep. If any length of time elapses prior to the reduction, either by the surgeon or by the efforts of nature, the hand becomes adducted, and there appears on the dorsal surface of the wrist and the neighboring parts of the forearm a painful swelling similar to that accompanying fractures, dislocations or sprains.

M. Goyrand had always considered the lesion in these cases as a partial dislocation of the radius at the elbow, but so slight as to give no appreciable deformity of the articulation. In 1857, a case occurred which revealed to him what he now conceives to be the true nature of the injury, viz., a displacement of the inter-articular fibro-cartilage of the wrist in front of the carpal extremity of the ulna. In complete pronation, the inferior articulating surface of the head of the ulna is exposed behind the corresponding border of the fibro-cartilage by more than three quarters the thickness of the bone, and hence, in a forced rotation of the radius in the direction of pronation, it is conceivable that the fibro-cartilage may easily be carried in front of the head of the ulna. This displacement occurs only in childhood, as it can only be produced by great violence; in

adults, forced pronation is prevented by antagonistic muscles brought into action as soon as pronation becomes painful. This cannot be done by a child. The varying size of the head of the ulna explains why some children, exposed to a cause capable of producing this displacement of the fibro-cartilage, escape the accident.

The following cases are submitted in justification of this diagnosis, and as illustrating the symptoms developed by the accident.

Obs. I. Mary J., one year old, just beginning to walk. July 6, 1861, whilst walking, being held by the right hand, her brother, seven or eight years old, fell heavily against her. The nurse, squeezing its hand, pulled the child quickly towards her, and in doing so felt something "give way" in the little arm. The child immediately began to cry; the limb was held motionless, resting against the body, carried a little in front, and with the fore-arm pronated. No deformity about the wrist or elbow. Motion or pressure about the elbow causes no pain, but pressure on the back of the wrist makes the child cry out. On supinating the hand, the movement was felt to bring up against some resistance, the child cried louder, and the hand when released assumed its previous pronated position.

The arm being held by the mother, M. Goyrand seized the hand in his right, with the thumb on the lower end of the radius and the fore-finger on the head of the ulna; with his left hand he gently supinated that of the child. Before supination was completely effected, the finger resting on the end of the ulna felt a slight shock; instantly the resistance to supination was overcome and reduction accomplished.

Two minutes afterwards the child took in its injured hand a bracelet held out to it by its mother. The mobility was completely regained, and all uneasiness and pain gone.

Obs. II. Margaret D., thirty-five months old, held by the left hand, made a false step, and from the sudden jerk which the hand in a state of pronation received there resulted the injury which M. Goyrand makes the subject of his paper. One of his pupils, a physician in the town where the child's parents reside, being called to the case immediately on its occurrence, recognized the injury and supposed that he had reduced it. The symptoms of the displacement did not, however, disappear, for on the following day the limb was immovable and in a pronated position. M. Goyrand saw the child fifty hours after the accident and noted the following symptoms, viz., those which are mentioned in the previous observation, and in addition, on the dorsal surface of the wrist, a swelling extending to the neighboring parts of the fore-arm and hand; this was extremely tender to the touch, especially at a point corresponding to the articular interspace of the wrist. There was a slight inclination of the hand to the ulnar side of the limb. In reducing the displacement, the finger which rested on the carpal extremity of the ulna felt, distinctly, when the hand was brought into supination, a crepitus precisely like that in a case of fracture. Before supination was completely produced the sensation ceased, opposition to supination was

overcome and the reduction was accomplished. Thirty-six hours afterwards no traces of the accident remained.

In both these children the displacement happened again in the same arm. In other cases which have been noticed, the two limbs have been successively the subject of the accident, one after the other.

In the discussion which ensued upon the presentation of this paper, it appears that MM. Guersent, Marjolin, Giraldes and Velpeau were at a loss to accept M. Goyrand's explanation. M. Guersent believed that the cases in question comprised a variety of lesions with certain identical symptoms. M. Marjolin thought that in many of these cases he had seen a deformity about the elbow. MM. Giraldes and Velpeau thought that a dislocation of the fibro-cartilage could not take place without a lesion of the serous membrane of the articular cavity, after which consecutive symptoms should be developed, no matter how simple the reduction might be. Are we then to accept this explanation of the phenomena presented by this injury?

Although M. Goyrand fortifies his opinion by the results of experiments on the dead subject, he admits that the fibro-cartilage will not remain displaced unless the hand is held up and in forced supination. In two attempts which I have made upon very young subjects, I have not been able to produce this dislocation, nor when the joint was opened have I been able to force the fibro-cartilage into the position which M. Goyrand assumes that it takes, without tearing it from its attachments. Any one who will examine for himself the very limited range of displacement of which this cartilage is capable, except by the use of violence which must determine more marked symptoms than ever follow this accident, will be convinced of the error of M. Goyrand's views. Although no two writers agree upon the precise nature of the injury, all of them, with the exception of M. Goyrand, locate it at the elbow. This diversity of opinion is accounted for by the fact, that before its seat can well be ascertained the symptoms disappear with the very first exploratory manipulations. It would seem, from the weight of testimony and from my own experience, that without attempting to account for this somewhat peculiar, and, as I judge, not unfrequent accident, by any ingenious theory or complicated displacement, a satisfactory explanation of its pathology is to be found in the partial luxation of the head of the radius, which, either in one direction or another, is accepted as its rationale by Duverney, Bouley, Monteggia, Martin, and Fougeu d'Etampes. This view finds support in the position assumed by the arm, and the manner in which the symptoms of the injury disappear. Rotation and flexion (which, of course, if carried through the whole range, implies extension) are the first motions which every surgeon gives to an arm when he examines it for a suspected fracture or dislocation; and in a child, where this bone was but partially out of place, might well be adequate to the reduction, even when practised gently and but a single time. An adult would perhaps unwittingly

or of his own accord reduce a dislocation of this slight degree, when in a child, from pain and fear, it might remain persistent till surgical aid arrives. In the absence of this, we are told that in the course of time spontaneous cure is effected. Would this occur if there were so serious a displacement as that of the interarticular fibro-cartilage of the wrist? It appears then that the following opinions present themselves.

1st. That of M. Guersent, who conceives that the cases in question comprise a variety of lesions with certain identical symptoms. 2d. That of a number of writers, who attribute the symptoms to a partial dislocation of the head of the radius in one direction or another. 3d. That of M. Goyrand, which has been detailed at length.

The cases present too striking a similarity to admit of the acceptance of the first of these, and of the third we have already given our opinion.

The second, therefore, seems to merit the most favorable verdict, and the latitude which it permits is, in the present state of our knowledge, no more perhaps than it is discretionary to allow.

Since writing the above, a third case has fallen under my notice. A child ten months old was brought to the dispensary, June 24th, 1862, with her hand in a sling, having received an injury to the elbow the afternoon previous, by being lifted up with a jerk by the wrists whilst lying on its back, in which position its grandmother had been putting on a diaper. The symptoms of injury, to wit, crying and inability to use the arm, ensued at once. On examination the following morning, the characteristic position, semi-flexion and pronation, and pain on motion, with the absence of deformity or swelling, led me to suspect the nature of the accident, and I was able to satisfy myself that the seat of injury was not at the wrist. The opposition to rotation was evidently at the elbow, and a single sensation of crepitus was felt at this point, but on manipulating to detect, if possible, the condition of the parts, the resistance to supination was unexpectedly overcome and the normal state of things assumed, before I had ascertained what the precise derangement was which stood in the way of free motion. The experience of this case confirms me in my opinion of the erroneousness of M. Goyrand's views, at least in their general application.

ON CATARACT.—No. II.

[Continued from page 93.]

To explain how it is that large pieces of the lens remaining in the eye after extraction may easily interfere with or prevent success, we must go back a little.

What is the state of things in an eye just after the lens has been entirely removed and extraction correctly performed? What changes have taken place? Two substances of considerable volume

have passed out of the eye, or rather, been pressed out, the aqueous and the lens. Estimating the average depth of the anterior chamber at 1 line and its breadth at 5½, the axis (of the lens) at 1½ lines and its equatorial diameter at 4 lines, and bearing in mind the shape of the spaces excavated, we can approximately estimate the loss of contents which the ball has undergone. It is relatively large and amounts to about the ninth or tenth part of the contents of the eye.

As soon as the aqueous has been pressed out of the eye, we see the pupil very much contracted, and the iris lying close on to the cornea and the lens close on to the iris. In general no air passes in to take the place of the aqueous, and the cornea is not pressed in by the atmospheric pressure, and not even perceptibly flattened. Nor does this in general take place when the lens is pressed out either spontaneously or by the operator.* The iris and vitreous come to occupy the place of the lens.

It is certain that a portion of the loss of contents of the ball is made up by an increased amount of blood in the iris, choroid and retina. But by far the greater portion must be otherwise accounted for. The vitreous cannot expand; consequently the cornea and sclerotic must contract upon themselves or be pressed together. The elastic cushion of fat which surrounds the ball posteriorly might indeed press the sclerotic a little towards the centre of the eye; but whether it really does so, can hardly be shown. * * * * * * The most probable theory is that the ball, somewhat compressed laterally by the muscles (recti-obliqui and orbicularis palpebrarum), becomes smaller in equatorial diameter. What has led Arlt to this view is the fact, that since he has had most of his patients lie down during the operation, the so-called collapse of the cornea has been very much more frequent, and the flowing-out of the vitreous extremely seldom. This naturally agrees with the view he has proposed, that the ball derives that degree of tension which it in normal condition presents to the finger, not so much from its being filled to the maximum with fluids, as rather from the tension and tone of the muscles which surround it on the outside, and of the ciliary muscle which keeps the line of union between cornea and sclerotic drawn inwards.

When the patients lie down during the operation, all the muscles seem to be in a lower degree of tension; the lens is then seldom pressed out of the eye spontaneously, that is, by increased tension of the muscles of the eye; it must almost always be helped out by the following method. The fingers of the operator exert a certain pressure on the ball, according to circumstances, at one time through the lower lid, then through the upper or through both at once. * * * * If now the iris and posterior capsule, after removing in a proper manner the whole of the lens, are pushed forward by the vitreous, then three layers lie somewhat concentric together; name-

* Note in the original.—"These remarks apply strictly only when the patient sits at the operation."

ly, the cornea, iris, and posterior capsule. Between the two latter towards the periphery lie the torn edges of the divided anterior capsule. In the attempts at seeing, the rays pass through the cornea, posterior capsule, and vitreous. If remnants of lens or of anterior capsule remain in the region of the pupil, they interfere with vision, partly by keeping out a portion of the light and partly by scattering a portion.

If remnants of the lens lie between the anterior and posterior capsule behind the iris, we can recognize the fact in two ways; first, by examining the size and form of the lens removed from the eye; with some practice we can recognize if larger pieces have peeled off. Especial regard must be had to the equatorial diameter and to the edge of the lens. But if the lens is very soft and has not come out whole, but in pieces, then this means of judging fails. Secondly, we examine the edges of the corneal incision to see if they are everywhere well applied to each other; if they are not so, it may be owing to large remnants of lens left in the eye, but also to other causes; we must be able to exclude the existence of the latter, to infer the presence of the former.

A bad corneal incision, especially a sawing one, would probably be unfavorable to the apposition of the edges. If the incision is made too extensive, in the conjunctiva, then a portion of the latter may fall between the edges of the wound (in which case it should be excised); or the peripheral portion of the iris falls forward into it and cannot be replaced, and must without hesitation be excised. (A small portion of the pupillary margin is brought forward and removed.)

Prolapses of the iris which are not occasioned in this way, may be replaced by gently stroking the upper lid and then suddenly opening the eye, or by lightly stroking with the curette. For the most part, however, it is only necessary to make trial of vision, the light causing the sphincter to contract and the prolapsed iris to recede.

The apposition of the edges of the incision may also be prevented by an eyelash accidentally getting between them, or by a clot of blood. Oftener it is a small particle of the lens which has remained in the wound.

When the above causes do not exist, and yet the wound does not close well, it may be inferred that remnants of the lens press forward the iris and cornea.

That a particular situation of the under lid with respect to the globe could prevent the apposition of the flap, seems to Arlt not at all demonstrated, although this circumstance has been cited in favor of the upper section as if it were a matter of course. The under lid may lift the flap, if the latter from other causes is not well apposed; otherwise, not.

If large pieces of the lens lie between the anterior and posterior capsule behind the iris, opposite the corneal flap, they are very likely to press forward the iris and also the corneal flap, as they

meet with less resistance in front than behind where the vitreous arches the posterior capsule forward. Then we must lay the index finger on the under lid, and partly by pressure through the lid and partly by stroking upwards, endeavor to bring the piece of lens into the pupil and get it within reach of the curette, or a piece of the iris (sphincter) must be excised. And yet the flap may be in perfect apposition, although remnants of considerable size remain in the fold between the anterior and posterior capsule. Sometimes, notwithstanding great care and attention to this matter, we have, after removing the bandage, been convinced that far more of the lens remained than we had suspected, and that even in cases in which the whole was thought to have been removed, very considerable pieces were left, sometimes occasioning disagreeable consequences (prolapsus iridis and iritis), and even loss of the eye (irido-chorioiditis). To this class belong many cases in which the cataracts were not perfectly ripe when operated on; not, however, from failure to recognize this fact, but from being urged to operate by other circumstances. As excuse for operating in such cases, it must be mentioned, that we find that a pretty large number result successfully, or at least, not unsuccessfully, even when the cataracts are unripe, and when considerable remnants (of ripe or unripe) are left behind in the eye; and by the above, it was not intended to say that every extraction performed before complete ripeness of the cataract must necessarily turn out badly, but only that one of the most frequent causes of an unsuccessful or a less favorable result than was hoped for, is owing to leaving behind in the eye pieces of lens of considerable size.

If the edges are well in apposition the union takes place in a short time. It is very probable that, after ten or twelve hours, the aqueous is again restored and remains so. At first, a few hours after the operation, the union is still so weak that, from time to time, some of the aqueous flows off. Subsequently this ceases (in normal cases); but if it continues, it is not without injury, or, at least, danger.

The bursting open of the wound at a time when the union ought to be firm, may be owing to various causes. Among them, one that has been too little considered is the pressure of remnants of the lens.

It is well known that when, after opening the anterior capsule, the fibres of the lens are brought into direct contact with the aqueous, they then become cloudy and subsequently swell out, finally become fluid, and are absorbed. Transparent fibres seem capable of much greater swelling after opening the capsule, than those which were already cloudy and swollen before contact with the aqueous. After the operation of discision, which consists merely in puncturing the cornea with a very fine needle, and making one or more incisions in the anterior capsule, we sometimes see, after a few days, and after the lens has become swollen, extremely dangerous symptoms—for instance, much redness about the cornea, chemosis of the

conjunctiva of the globe, hemicrania, and even vomiting, &c. Now-a-days it is known that these symptoms are owing to increased intraocular pressure arising from the swelling of the lens, and that they can be controlled, if at all, not by bleeding, cold lotions, mercurials, &c., but only by atropine, letting off the aqueous, iridectomy or linear extraction, together with or without iridectomy. They can best be avoided by making only a small opening in the capsule, and by taking care not to break in pieces the lens itself as was formerly advised. In this way we best prevent the too sudden swelling of the lens. If we apply to the case of extraction what is observed in discision, we can anticipate what may take place if portions of the lens of considerable size remain, with the large opening in the capsule, and supposing the aqueous to continue restored. Clinical observation has furnished facts which accord with this supposition, or rather, which find their explanation in the theory obtained by observing the operation of discision.

[To be continued.]

HOSPITAL REPORT.—THE SACCHARINE TREATMENT OF DIABETES MELLITUS.

By John Hughes, M.D., Senior Physician to the Mater Misericordiæ Hospital.

The saccharine plan of treating diabetes originated, I believe, in France, and so far back as 1845. Bouchardat gave saccharine fruits in diabetes, and bread made from gluten. Andral and Piorry tried a similar treatment, with some success. And the practice has been recently adopted in England, by Dr. Budd, of Bristol, who has published some cases (two, I think) in which he says the most marked amendment followed the use of sugar. Others have also recorded cases; but they do not exhibit so favorable a result as those of Dr. Budd. It would appear, however, that some practitioners who tried this plan have found it beneficial. Their patients grew fat upon it; and even this effect, in a disease where wasting is so prominent a symptom, is a very desirable result. They say, besides, that the practice is not an irrational one; for Bernard has shown that sugar taken into the stomach, in its passage through the liver, is converted into an emulsive substance, which tends to fatten patients; and he has also proved experimentally, and Andral and others practically, that sugar is secreted and found in the circulation in diabetes, whether the individual be fed upon nitrogenous or amylaceous substances: consequently our old-established plan of dietetics in this disease, with all its restrictions, is useless.

Dr. Budd, one of its earliest advocates in England, says he gives sugar in diabetes on the principle of supplying to the system the particular element which is running to waste, and the loss of which appears to be the principal cause of the damage sustained by the constitution as the disease advances. On all these grounds, then, it

has been considered that the saccharine treatment of diabetes is worthy of a trial.

Amongst the many theories propounded concerning this intractable disease, modern researches incline to the opinion that the liver is the organ in fault—an idea long since entertained by Dr. Prout. Experiments by Bernard and Pavey tend to show that there is always present in the liver, located in the hepatic cells in considerable abundance, a substance which one calls the "glucogenic matter" of the liver, the other "hepatine;" that this substance is, with great facility, by a process allied to fermentation, converted into sugar; but that it seems to have the power, whilst located in the tissues of the living and healthy liver, to resist the transformation. In certain unnatural conditions, however, as well as after death, this power is at an end, and the blood becomes surcharged with saccharine principle.

If this be true, it would appear that the diabetic condition depends upon some functional derangement of the liver, which converts alimentary substances into this glucogenic matter in greater abundance than natural, and allows it to mix with the blood in large quantity, when it immediately becomes converted into sugar, and as such passes off with the urine.

I am not sure, even assuming all this to be correct, whether we are in a better position to decide upon the exact nature of this disease. Probably, however, it is to physiology we shall have to look, in the end, for a solution of this difficult question; for pathology is strangely barren of results in this disease; so much so that it is doubtful whether the presence of sugar in the system either necessarily depends upon, or produces, visible organic lesion of any particular organ. When organic diseases do exist, they are looked upon as merely concurrent affections.

However, it is not my intention to discuss the nature of diabetes mellitus; I merely wish to give a brief account of how this saccharine plan of treatment turned out in my hands, after a trial of more than four months.

Four cases of diabetes mellitus came under my care in hospital, almost simultaneously.

The first was a man named Thomas Ryan, aged 37, who had been diabetic for 13 months before admission, and had been under treatmen for his disease during the greater part of that time. On admission he was voiding, daily, eight pints of urine, specific gravity 1049, and containing 22 grains of sugar in each ounce. He complained of great thirst, languor, and debility; the skin and mucous membrane were dry; the bowels confined; and all the usual symptoms of diabetes were present.

I treated this man with Dover's powder and the vapor bath, for a fortnight, when he left the hospital relieved in respect to the condition of the skin and mucous membrane; his thirst was abated, and the skin was somewhat moist; the quantity of urine varied with

the amount of fluid drunk, but its condition was unaltered. He thought he was growing weak, and wished to go home. He told me his father had a complaint similar to his own.

This man returned on the 18th January, and was then voiding 10 pints of urine daily, of a specific gravity 1041, 24 grains of sugar in each ounce. He said he drank a large quantity of beer, one day, at home, and was not as well since. I now determined to put him on the saccharine treatment, and ordered him six ounces of barley sugar daily; diet of fresh meat, with green vegetables and bread; also a moderate quantity of lime-water and milk. He continued this plan steadily for three weeks; and at the end of that period his condition was, to a certain extent, improved. The quantity of urine passed was seven pints, the specific gravity 1041; each ounce contained 24 grains of sugar; and he had gained two pounds in weight. The skin was somewhat moist, thirst abated. He was again anxious to return home, and left the hospital.

The second case was a man, aged 40; but as he was not in hospital more than a week, and was treated with sudorifics (Dover's powder) alone, and almost an exclusively animal diet—I will only refer to his case. In fact, he would not submit to the abstinence from fluids, and the variety of food which I enjoined. He left without any apparent change. There was one fact connected with him of interest—he told us his father had the same ailment he was laboring under, and died of it.

The next patient was a man aged 32 (John O'Neill), who suffered from the complaint for 18 months before admission. On the 3d January he was voiding 15 pints of urine, of a specific gravity 1043, 18 grains of sugar to the ounce. He was very thin, and had all the symptoms of diabetes in an aggravated form. He was treated with sugar and a mixed diet, like the former patient; and at the end of six weeks his urine was reduced in quantity to six pints—the specific gravity remaining the same. All the other symptoms were greatly relieved, and he felt himself much better and stronger; in fact so well that he was anxious to go and resume his former employment (that of a shopman). Yet, on weighing him, we found he had lost four pounds in weight since his admission, and his urine contained 22 grains of sugar to the ounce. We heard that he has since died of phthisis.

The last and most interesting case is that of Henry M'Nee. He was a married man, 30 years of age; tall, well-proportioned, and of a very athletic frame. He was always temperate; had no hereditary predisposition to the disease, and attributes his illness to profuse perspirations and alternate chills while working as a railway laborer. Five years ago, when employed in England, he first noticed his disease, and was treated for it at the Manchester Infirmary. After four months stay in that institution, he left at his own request, relieved sufficiently to resume his work, at which he continued for 11 months before admission. At that time he noticed the

aggravation of his disorder, which set in with great thirst, increased flow of urine, general weakness, and rapid loss of flesh.

On admission, all these symptoms had attained a great intensity. He said he was only the skeleton of his former self; for, when in health, he weighed more than 14 stone, and now he did not reach 12; which surprised him, when he could eat so much—four times his ordinary quantity—and he did not feel sick, only very weak. He was voiding 10 quarts of urine in 24 hours, of specific gravity 1049, and was obliged to empty the bladder every hour. He drinks about the same quantity of fluids within the same time. His urine has an acid reaction, is free from albumen, and each ounce contains 24 grains of sugar. As an evidence of his broken down health we found a large, chronic, indolent ulcer over the right external ankle.

I was determined to give the saccharine treatment an uncomplicated trial in this case; and, after an aperient, I put the patient on six ounces of sugar, daily, together with four ounces of treacle; bread, meat and green vegetables for diet; lime-water and milk for drink—with an injunction to limit the amount as much as possible.

At the end of a month he was somewhat improved. He had gained two pounds in weight; his thirst and appetite were diminished; the quantity of urine passed in 24 hours was reduced from 10 to 7 quarts; the specific gravity ranged from 1043 to 1045—26 grains of sugar to the ounce.

During the next month he had two attacks of sudden and violent sickness of stomach, accompanied with constant vomiting and cramps in the abdomen and legs. He complained, for a few days, of great nausea, and felt as if saturated with sugar; everything, he said, tasted sweet. He was, at the same time, weak. The urine was of a specific gravity of 1044—not lessened in quantity. The ulcer of leg was healed. The sugar treatment was discontinued.

After the lapse of a few days the sugar was again resumed; and his condition, at the end of another month, was as follows:—His weight, 12 stone 11 lb.; consequently he had gained nine pounds since last report. His urine is reduced to three quarts in 24 hours; and he is not disturbed more than once or twice to pass it during the night. His skin is moist; his bowels are regular; he has gained strength, for he is able to work at the force-pump of the hospital for an hour without resting. The specific gravity of the urine is 1035–9, but it contains a *greater amount* of sugar than before. According to Garrod's glucometer each ounce contains 40 grains of sugar. His appetite and thirst have decreased; the ulcer of the leg has broken out again.

After four months stay, he left the hospital in the month of May, and obtained employment as a porter, which obliged him to carry considerable weights; he remained at this work for six months, during which time I saw him occasionally; but at the end of that

period he was completely prostrate, and sought relief in another hospital. As the sequel of his case has been published, I will add some extracts from the report:—

He was admitted into Dun's Hospital, under the care of Professor Law, in the month of January, and was then voiding 16 pints of urine in 24 hours—specific gravity 1042. On the 10th of February the quantity of urine was 12 pints—specific gravity 1035–9, and contained 8·750 grains of sugar, or about 45½ grains to an ounce. On the 8th of March the quantity of sugar was 39 grains to the ounce, the amount voided being the same. On the 20th March the quantity of sugar declined to 34 grains; and on the 8th May the urine was reduced to 10 pints, and there were 38 grains of sugar in each ounce.

He left the hospital in July, but was again readmitted late in October, in an advanced stage of phthisis; and on the 10th November the *post-mortem* examination showed extensive tubercular disease in both lungs. "Both kidneys were very large; one weighed 12½ ounces, the other 11. Both were much congested, but exhibited no trace of disease or deviation from their normal structure. The liver was perfectly normal in size and appearance; and, on examination, did not contain a trace of sugar. It was, in fact, to the eye and to chemical analysis a specimen of a healthy liver."

It will be seen from these cases in which the saccharine treatment has had a pretty fair trial, that, to say the least, it produced no permanent improvement. The specific gravity of the urine was not altered, and in each instance its saccharine quality was aggravated. 'Tis true the amount of urine voided within a given period was considerably diminished; but I think that result is very much within the control of the patient, exclusive of medicine. I mean, of course, if he checks his desire for fluids. The gain in weight and the increased strength may be more justly attributed to other causes than to the amount of sugar taken; and I am quite satisfied, so far as my observation enables me to judge, that the saccharine treatment of diabetes is not entitled to the credit which its advocates claim for it. All that can be said for it is, that it is vastly agreeable to patients, and is not positively injurious, as one might *a priori* be inclined to suppose.—*Dublin Quar. Jour. of Med. Science.*

The Astley Cooper Prize of 1500 dollars has been awarded to Dr. Edward Crisp, by the physicians and surgeons of Guy's Hospital, for his essay on Anatomy, Physiology, and Pathology of the Human Pancreas.

British Medical Association.—The thirteenth annual meeting of this association was held in London on the 5th, 6th, 7th and 8th of August. We see by our late journals that the meeting was largely attended and was a highly interesting one, and that Professor Wood, of Philadelphia, and Dr. Ephraim Cutter, of Woburn, Massachusetts, were present.—*Medical News.*

Reports of Medical Societies.

EXTRACTS FROM THE RECORDS OF THE BOSTON SOCIETY FOR MEDICAL IMPROVEMENT. BY FRANCIS MINOT, M.D., SECRETARY.

Aug. 25th.—*Lumbar Abscess; Early Opening; Speedy Recovery.*—Dr. Abbot read the following account of a case, which he had received from Dr. Stephen Tracy, of Andover.

"The patient, Mr. W. H., age 30: married: stature rather below the average height, and rather thick set; hair and beard very dark: skin soft and thin; cheeks full, florid: muscular motions very quick; occupation shoemaking, mostly: habits strictly temperate.

"In December, 1859, he suffered from pain in left lumbar region, regarded at the time as rheumatic. Was then under treatment about two weeks. Has occasionally experienced pain there since, but has been otherwise well, and has labored hard.

"On the 7th of March, 1862, I was called to see this patient. He said that for several weeks he had felt something of that old pain in his side, and yet it was not exactly the same as he had before. It had now become so severe that he must have medicine.

"At first I regarded it as rheumatic, but after some ten or twelve days I was led to suspect lumbar abscess from the characteristic drawing up of the knee: irritative or hectic fever, followed by profuse perspiration;" severe, sharp pain, &c. In the course of a few days, the painful region was found to be slightly swollen, and the patient complained of a peculiar numbness in the centre of the diseased part.

"After a few days a tumor could, by hard pressure, be distinctly felt; its centre being midway between the crest of the ileum and the ribs, and under the anterior border of the tendinous expansion of the latissimus dorsi muscle. The drawing upwards and inwards of the knee rather increased, and so did the fever. In short, there was every evidence of the existence of an abscess except the feeling of fluctuation (and the tumor was too deep for that), and the actual seeing of the pus. The constitutional symptoms increased: appetite nearly gone, and the patient was losing flesh and strength rapidly. Yet he was able to sit up in a chair much of the day, but it was becoming morally certain that he would ultimately sink, unless relief could be soon afforded.

"This state of things led me to seriously call in question the wisdom of surgical writers in regarding lumbar abscess as forming an exception to the general rule to open abscesses situated under fascias early. I could think of no sufficient reasons for so doing, and resolved upon using an exploring needle, or trocar rather, and if I found pus to give it exit. Accordingly, on the 31st of March, twenty-four days from the time of my first visit, having etherized my patient, I introduced a small exploring trocar into the central part of the tumor. It was not till the point of the instrument had reached the depth of three inches, within a line or two, that diminished resistance evidenced its entrance into the cavity of the sac. I then withdrew the stilette, but no fluid followed. Feeling positive that I had penetrated a cavity containing pus, and thinking it might be too thick to flow through so small a canula, I applied my mouth, and by repeated strong suction drew out laudable pus. My diagnosis was now beyond the possibility of doubt. I then introduced along the track of the canula a narrow (three-sixteenth inch wide) bistoury into the sac of the abscess, then pressing

the point of the instrument forward, made the inner extremity of the opening a very little larger than the outer. I then turned the instrument, and pressing upon the point, made the inner portion of the incision larger than the outer by cutting the fibres of the fascias at right angles: then, turning the instrument to its first position, I withdrew it. A few drops of thick pus followed. I then introduced a grooved director from my pocket case, four and three-fourths inches before meeting with the least resistance. I then withdrew the instrument about an inch and a half, and made firm lateral pressure with the point of the instrument in all directions, with the design of thus preventing adhesion taking place, particularly at the inner part of the opening, before the pus should be discharged. I then withdrew the instrument, leaving the external opening only of the size necessarily made by the narrow bistoury used.

"A poultice was now applied and I left my patient, expecting that I had made such an opening through the several fascias as would enable the vital powers, before many days, to make an exit for the pus without any risk of the absorption of atmospheric air into the cavity of the sac.

"In this I was not disappointed. There continued to be a discharge of a drop or two of pus from time to time for five days, when it became sufficiently free; from one to four or six ounces or more escaping at a time on several occasions. No flocculi were discovered in the pus, but it became more thin and watery towards the last. The discharge continued in gradually diminishing quantities for about three weeks, when it entirely ceased. I think there could not have been less than two pints, and the family say there was certainly more than three pints discharged in all.

"The swelling, of course, diminished rapidly, and so did the drawing up of the knee as well as the irritative fever, while the appetite and strength as rapidly increased. By the first of May he was able to be out upon the street, and by the first of June was able to work half the day without injury, and has continued improving without any drawback, till now, Aug. 22d, he considers himself *entirely* well; and, so far as I can see, is well, and has been so for more than two months.

"The treatment throughout was anodyne, alterative and sustaining. About three weeks after the opening, I commenced the application of the ethereal tincture of iodine to the part, extending it over the whole left lumbar region, with evident benefit in forwarding the cure. It very perceptibly increased the quantity of urine.

"Query 1st. Have I probably seen the last of it?

"2d. Did I reason and act wisely in opening it as early as I did, and in the manner I did?"

Dr. Gay thought there was some doubt as to whether the case really were one of lumbar abscess, since there were no symptoms referable to the vertebral column, and the pus was laudable, instead of being thin and curdy, as is usually the case in lumbar abscess.

Dr. Hodges observed that a "lumbar abscess," that is to say, an abscess dependent on some lesion of the vertebræ, does occasionally point in the lumbar region, yet its ordinary course is along the psoas muscle, and within its sheath, to the front of the thigh, whence its common name. Considering, therefore, the rarity of the former occurrence, the acute character of the symptoms characterizing the present case, the absence of constitutional disturbance on letting out the

matter, and the subsequent history of the patient, the presumption seemed to be rather in favor of an idiopathic abscess in the lumbar parietes. The puncture was probably made in the region of the quadratus lumborum, and, not near the psoas muscle, which, indeed, could hardly be reached, if the opening were made from behind. The depth to which the trocar penetrated was probably less than the reporter imagined. Abscesses of the iliac fossa have been opened in this region, and M. Nélaton observes, that the thickness of the soft parts which the knife passes through, is not great enough for the operation to present any difficulty.

Dr. Gay thought that most surgeons would have hesitated before plunging a trocar to the depth of three inches in that direction, without coming to any matter, unless there were great external swelling and fluctuation.

Aug. 25th.—*Fatal Apoplexy; Mass of Cholesterine in the Gall-bladder.*—Dr. C. E. Ware showed a mass of nearly pure cholesterine of a perfectly oval form, about the size of an almond with the shell on, which was taken from the gall-bladder of a man who died of apoplexy. The patient was a shoemaker, about 58 years old, previously healthy, who was found lying beside his bench, insensible, shortly after he had dined, having vomited copiously. He was breathing heavily: there was no distortion of the features: the pupils were slightly dilated and fixed. There was no voluntary motion of either extremity. The respiration was alternately calm and labored. The man died about six hours from the time of his attack. At the autopsy, a large extravasation of fluid blood was found under the membranes of the brain, and between the convolutions under the right temporal bone, in the region of the lateral sinus. No blood in the ventricles. In the substance of the right side of the pons Varolii was a considerable effusion of blood, partly coagulated: a similar effusion, rather less in extent, in the left side of the pons. The upper part of the left side of the pons, in the neighborhood of the fourth ventricle, was a good deal broken down, and the upper part of the left hemisphere of the cerebellum was a good deal softened. The general appearance of the brain was healthy, though somewhat flabby and pale. The arteries of the base of the brain were accidentally not examined.

The thoracic viscera were healthy; the cavities of the heart were all entirely empty.

The gall-bladder was filled with a thin, semi-transparent fluid. Encysted in its neck was the mass of cholesterine mentioned above.

The capsule of the right kidney was somewhat thin, but adherent, so that portions of the kidney were torn away where it was removed. Externally it was granular in appearance, and of a slightly yellowish cast, masked by engorgement with blood. The organ contained two or three small cysts filled with a pale fluid. For the extent of one half the kidney, the pyramids, and all traces of renal structure, were obliterated. In the remaining half the calices were dilated. The microscope showed oil globules and broken-down tubules in abundance. The left kidney was nearly healthy in appearance.

THE BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON: THURSDAY, SEPTEMBER 18, 1862.

MANUAL EXTENSION AS A SUBSTITUTE FOR TENOTOMY FOR THE CURE OF DEFORMITIES FROM MUSCULAR CONTRACTION.—Of late years, tenotomy has got into such extensive use for the removal of this class of deformities, as to take the place of almost all other modes of treatment. It is therefore with much interest that we have read in the *Journal de Médecine de Bordeaux* an account of Dr. Larghi's successful management of these affections by manual extension alone. We translate the notice in full, believing it will interest our readers.

Tenotomy for the cure of muscular contractions has been considered, up to the present time, as constituting a genuine advance in the treatment of this class of affections. The Surgeon-in-Chief of the Hospital of Vercelli, however, Dr. Bernadino Larghi, has declared himself in opposition to this method of treatment. When a part is contracted, shortened, says he, what should be done? It must be lengthened. It is then to extension, not to incision, that we should have recourse. The surgeon acts like a harpist, who, to elongate the string of his instrument which is too tense, divides it. This is a fundamental error. This fact is so evident to the author, that he thinks it needs no demonstration. Tenotomy to him has been a step backward in surgery. To-day there is a slight reaction from the furor with which tendons and aponeuroses were divided; but if there is a temporary pause, the means have not yet been discovered by which the affections for which tenotomy has been employed may be cured without it. This method is very simple; it is manual extension, which Dr. Larghi has employed for years (*da anni ad anni*); momentary extension, made morning and evening or even but once a day.

It must not be thought that the author is ignorant of the condition of the affected parts, the tendons, mucsles, &c. of the shortened members, nor of what happens after tenotomy. But it is not only at the place for incision that he thinks it necessary to practise extension, where he might obtain a partial elongation, but to be effectual it must be practised in the whole course of the fibres implicated; and manual extension does this. It is by the normal muscular action, the action of contraction and lengthening, that we must hope to overcome these contractions. The contracted parts are atrophied, and it is only extension which will restore their length, the movement rousing them to new life, giving them increase of vigor to overcome and diminish to a great degree the atrophy.

After thus announcing the results which he hopes for from extension, Dr. Larghi explains his manner of employing it. It should last for a few moments only, be very gently applied, and very slowly. He afterwards examines its effects on all the parts implicated, muscles, tendons, aponeuroses, arteries, veins, nerves, and lastly the joints. He supposes a case of tenotomy practised for contraction of the thigh or at the knee, and compares it with the manual operation which has succeeded admirably with him since he renounced tenotomy and has had recourse only to extension or manual flexion.

Dr. Larghi has before strongly recommended the treatment by ex-

tension in the *Journal of the Royal Medico-Chirurgical Society of Turin*, for April, 1858. He had employed it with success in club-foot and contractions of the hand upon the forearm. There is, notwithstanding, a case in which he leaves it an open question whether extension is preferable to tenotomy; it is where there is *excessive* contraction of the hand upon the forearm. But in the cases where there is atrophy of the hand, arm, shoulder, together with the corresponding part of the trunk and head, it is useless to think of extension, and tenotomy is even more powerless.

Before tenotomy was practised, apparatus was employed; but this was an error, not of principle but of *modus faciendi*. The apparatus acted too powerfully and in a permanent way, whereas manual extension is an intelligent act, combining extension and flexion.

It was after employing tenotomy with more or less success that Dr. Larghi was led to employ extension, trying it in cases of contraction of the leg upon the thigh caused by the flexor muscles, of the forearm on the arm, the thigh on the pelvis, the arm upon the shoulder, and the shoulder on the trunk. By degrees he extended the application of his method, and finished by employing it in cases of club-foot.

In his published articles the author explains his manner of operating in each case; giving the details, which are full of interest. The extension should be repeated every day, so as to put in practice the adage of the imitator of nature, *nulla dies sine linea*. He does not pretend to say absolutely that he can act upon bony deformities, but he believes that he can modify them very advantageously. Perhaps he would have done better, had he, instead of contenting himself with mentioning his success in general terms, given in detail some of these facts. The following is a case of a patient still under treatment, and which is full of interest.

A young child was affected with varus of the right foot, and contraction of the left hand, so excessive that the atrophy involved hand, forearm and arm to the shoulder, the trunk, and the neck: the muscular and bony systems being both involved. The left hand is inclined towards the cubital side, and the palm towards the palmar surface of the forearm. This hand, which is incapable of any movement, cannot sustain the least weight, neither can the left arm be raised. By a series of manipulations some improvement has already been gained in the club-foot, the child being able to walk in the ordinary way, having previously dragged the foot after him, turned inward. The following is Dr. Larghi's method of extension. The patient is laid upon his belly and chest, face downwards. Holding the thigh firmly down at the tibio-femoral articulation, with one hand he seizes the lower extremity of the leg, and extends it little by little. Passing next to the foot, fixing the plantar surface against the operator, the heel being up and firmly held, he carries outwards the anterior and inverted extremity, and afterwards applies extension to the plantar surface itself. After a few repetitions of this operation the club-foot was entirely cured, and at the present time the patient walks with the right foot like the left, without using a cane or limping.

In operating on the hand he applied extension to the forearm with the dorsal surface turned upwards. An attendant held the hand, while another held the upper extremity, the palmar surface being turned downwards. The two assistants then made extension and counter-extension. The fingers, which were also contracted, were at

the same time drawn upon. The surgeon applied pressure upon the back of the wrist. This treatment was kept up for some time, the extension being repeated at short intervals: afterwards the extension was made at longer intervals. The arm, which at first could not be raised, can now be easily carried above the head, and both arm and shoulder have acquired sufficient movement, and the complete cure is certain. The contraction of the hand is less strong, and it is certain, that although it is turned inwards, the patient is able to open it sufficiently to seize and hold a light weight, which he could not do at first. It may be asked whether in this case more might have been gained by tenotomy. There can be no doubt of the answer.

We believe Dr. Larghi has opened a way in which others may follow; and we advise our brethren who may wish to obtain more of the details of his operations, to read the Memoir which the author has published in the *Gazetta Medica Italiana*, January, 1862.

The Frozen Well in Vermont.—A Committee of the Boston Society of Natural History, Dr. C. T. Jackson being Chairman, have made a report on the frozen well of Brandon, Vt., in which they say—

"Although we do not feel that we have been able to remove all doubts as to the true theory of the phenomena of the frozen well, still we incline to believe that the freezing is due to the nature of the conducting medium in which the well exists, and that the wave of heat in the summer months is not adequate to overcome the cold of the longer cold months, while the uncommonly severe winters of 1856 and 1857 may have lowered the temperature of the rocky masses of boulders, so that the wave of summer heat has not yet been able to reach the frozen mass, which, once congealed, would resist thawing on account of the slow conduction of ice. It should also be remembered that water does not conduct heat downward readily, though it does upward by convection."

Iridectomy in Glaucoma.—The Editor of the *American Journal of Ophthalmology* says,—In allusion to an interchange of statements by Dr. J. H. Dix of Boston, and Dr. Bumstead of New York, on the subject of performing iridectomy in glaucoma, and as to the priority of its performance in America, the Editor takes pleasure in stating that he —arrived in this country on the 4th of January, 1861—performed iridectomy in a case of chronic glaucoma in the beginning of February, and since. It will thus be seen that he performed the operation as early as it was well possible, and among the first.

"Furthermore, our results have been such as to confirm our previous views concerning its utility—views that we cannot change, even though, 'at the New York Eye Infirmary, Hancock's operation is preferred to Graefe's, as avoiding *deformity*.' [?!]"

We understand that the "Hand-book of Surgical Operations," by Dr. Stephen Smith, of New York, lately issued by Bailliere Brothers, very soon passed to a second edition, which has been already taken up, and that a third edition will shortly be in readiness. The work is well adapted to the more pressing wants of the profession at this time, and we are glad to learn that it is having so extensive a circulation.

PROVIDENCE, R. I., Sept. 13, 1862.

Mr. Editor,—I send you the following extract from a letter recently received from my friend, John Stanton Gould, of Hudson, N. Y. The gentleman, though not a medical man, is entirely competent to observe what he relates: his statement may therefore be implicitly relied upon. He met with the specimen during a recent visit to the Adirondack forests. The case seems to me worthy of record in the JOURNAL.

Yours truly, G. L. COLLINS.

"Two years ago a hunter, living near the head of Long Lake, shot a deer through the brain. On cutting it up, he found a curious appearance about the heart, which he cut out and preserved, first in salt and afterwards in spirit.

"The deer had previously been shot with a rifle ball, which had entered the chest, passed through the left auricle, diagonally through the left ventricle, the septum, and stopped in the outer wall of the right ventricle. The opening in the septum was closed by a membrane, as was that in the ventricle.

"After such a wound the deer recovered, and when shot, was fat and in splendid condition in all respects."

HEALTH OF PROVIDENCE, R. I.—From Dr. Snow's monthly report for August, it appears that the remarkable low rate of mortality in that city during July was not continued in the succeeding month, although the public health was still favorable—the deaths not exceeding the average number for the month of August in former years. The whole number of deaths for the month was 109—giving 1 in 464 of the inhabitants. Under 5 years, 60; between 80 and 90 years, 6; between 90 and 100 years, 3. Of consumption, 16; cholera infantum, 29.

40th Mass. Reg't—Dr. O. A. Brewster, Surgeon, and Drs. J. Cass and Andrew Smith, Assistant Surgeons. 41st Reg't—Dr. A. H. Blanchard, Surgeon, and Dr. J. Blackmer, Assistant Surgeon.

VITAL STATISTICS OF BOSTON.

FOR THE WEEK ENDING SATURDAY, SEPTEMBER 13th, 1862.

DEATHS.

	Males.	*Females*	*Total.*
Deaths during the week,	32	41	73
Average Mortality of the corresponding weeks of the ten years, 1851-1861,	49.7	49.9	99.6
Average corrected to increased population,	..	..	111.16
Deaths of persons above 90,	..	0	0

Mortality from Prevailing Diseases.

Phthisis.	Chol. Inf.	Croup.	Scar. Fev.	Pneumonia.	Variola.	Dysentery.	Typ. Fev.	Diphtheria.
10	13	2	1	2	0	3	2	0

PAMPHLETS RECEIVED.—Variola; its Nature and Treatment. By Andrew Nebinger, M.D., Philadelphia. —Medical Testimony in regard to the proper Mechanical Treatment of Joint Diseases. By Henry G. Davis, M.D., New York. Report of the Surgeons of the New York Ophthalmic Hospital, for the years 1860-61.

MARRIED,—At Acworth, N. H., on the 11th inst., by Rev. Amos Foster, S. M. Dinsmoor, M.D., of North Branch, to Miss Georgiana Carey, of Lempster.

DIED,—At Chelsea, on the 21st August, Dr. Alpheus Proctor, aged 57.

DEATHS IN BOSTON for the week ending Saturday noon, September 13th, 73. Males, 32—Females, 41. —Apoplexy, 1—inflammation of the brain, 1—burns, 1—cholera infantum, 13—consumption, 10—convulsions, 2—croup, 2—debility, 1—diarrhœa, 5—dropsy, 1—dropsy of the brain, 4—dysentery, 3—erysipelas, 3—scarlet fever, 1—typhoid fever, 2—disease of the heart, 2—hydrosis, 1—infantile disease, 6—intemperance, 1—disease of the liver, 1—inflammation of the lungs, 2—marasmus, 1—measles, 1—old age, 3—tonsillitis, 1—unknown, 4.

Under 5 years of age, 39—between 5 and 20 years, 5—between 20 and 40 years, 15—between 40 and 60 years, 6—above 60 years, 8. Born in the United States, 55—Ireland, 13—other places, 5.